Research Policy 42 (2013) 76–89

Contents lists available at SciVerse ScienceDirect

Research Policy

journal homepage: www.elsevier.com/locate/respol

Value for money? New microeconometric evidence on public R&D grants in Flanders☆

Dirk Czarnitzki[a,b,c,*], Cindy Lopes-Bento[a,c,d]

[a] K.U. Leuven, Department of Managerial Economics, Strategy and Innovation, Naamsestraat 69, 3000 Leuven, Belgium
[b] Centre for R&D Monitoring (ECOOM), K.U. Leuven, Leuven, Belgium
[c] Centre for European Economic Research (ZEW), Mannheim, Germany
[d] CEPS/INSTEAD, Esch-sur-Alzette, Luxembourg

ARTICLE INFO

Article history:
Received 28 March 2012
Accepted 20 April 2012
Available online 15 May 2012

Keywords:
R&D policy
Subsidies
Treatment effects estimation

ABSTRACT

A significant amount of money is spent on programs to stimulate innovative activities. In this paper, we review the effects of a specific government-sponsored commercial R&D program from various angles. We start by evaluating whether we find positive effects of subsidies on R&D investment and R&D employment. Then, we analyze how the observed effects of subsidies on R&D intensity and employment vary over time, vary if the firm receives also support from other sources, vary depending on how many supported projects a single firm has at the same time or vary if a firm gets support consecutively. Finally, we estimate the macroeconomic impact of these grants in terms of R&D employment. We conclude that (i) the policies are not subject to full crowding out, (ii) the treatments effects are stable over time, (iii) receiving subsidies from other sources in addition to the program under evaluation does not decrease the estimated treatment effect, and (iv) receiving grants repeatedly does not decrease the magnitude of the treatment effects either. Using a back-of-the envelope calculation, we estimate that, on average, five R&D jobs are created (or maintained) per supported project in the Flemish economy.

1. Introduction

The impact of subsidies on firms' innovative behaviour has been of interest in economic literature for many years now. In line with this literature, we are interested in knowing what the effect of one specific instrument is on firms' R&D intensity and R&D employment, namely the effect of subsidies for R&D from the Flemish government (northern part of Belgium). We employ econometric treatment effects models for estimating the treatment effect on the treated. As studies like this are nowadays more or less standard in the scholarly literature and even in policy practice, we go beyond the typical application of treatment effects models. Usually scholars estimate a treatment effect on the treated (see e.g. the survey by Cerulli, 2010), and then conclude whether a subsidy program is subject to full or partial crowding out effects. In this present study, we add a number of further tests that are of interest for policy makers in their daily decision making. The analyses presented in this paper are based on detailed discussions that the authors had with the representatives of the public agency administering the innovation policy instruments in Flanders, the "IWT Vlaanderen". In particular, the policy makers were interested in the following questions: Knowing from earlier evaluations that the estimated treatment effects are positive (see Aerts and Czarnitzki, 2006), it has been of primary interest whether

- the estimated treatment effects vary over time;
- the receipt of subsidies from other sources on top of IWT grants reduces the effect of the local policy program;
- funding the same firm repeatedly creates an increased risk of crowding out effects;
- and whether granting multiple projects to the same recipient firm in the same time period increases the risk of (partial) crowding out.

In addition to the questions mentioned above we also show that the treatment effects remain stable across different samples of firms i.e. using

i. a representative sample of firms in the Flemish economy (the "full sample")
ii. a subsample of firms that at least indicated some propensity to innovate (the "sample of innovators")

☆ Financial support from the IWT (Agency for Innovation by Science and Technology) as well as from the "Fonds National de la Recherche" (FNR), Luxembourg, is gratefully acknowledged.
* Corresponding author at: K.U. Leuven, Department of Managerial Economics, Strategy and Innovation, Naamsestraat 69, 3000 Leuven, Belgium.
E-mail address: Dirk.Czarnitzki@econ.kuleuven.be (D. Czarnitzki).

0048-7333/$ – see front matter © 2012 Elsevier B.V. All rights reserved.
http://dx.doi.org/10.1016/j.respol.2012.04.008

iii. a subsample of small and medium-sized firms, as a popular subscheme in Flemish innovation policy is a program foreseen for grants distributed to small and medium-sized firms (the “KMO program”).

As we have quite detailed information on innovation project grants in Flanders, we are also able to conduct a back-of-the-envelope calculation on how many R&D jobs are created in the Flemish economy because of subsidies to the business sector.

The following section will provide an overview on the institutional background and functioning of the funding agency. Section 3 reviews the existing literature and the undermining theory. In Section 4, we present the econometric method. Section 5 provides information on the data. Section 6 shows the econometric results and Section 7 provides information on the macroeconomic effect of the Flemish innovation policy on the Flemish economy. Section 8 concludes.

2. Institutional background[1]

2.1. The IWT

The agency for Innovation by Science and Technology in Flanders (“Innovatie door Wetenschap en Technologie in Vlaanderen” (IWT)) is a governmental agency, established by the Flemish Government in 1991. It was established to give shape to the new competences in science and technology that were transferred from the federal to the regional governments in Belgium. Since this transfer of competences made of innovation policies a regional matter, the IWT has been created as the key organization for support and promotion of R&D and innovation in Flanders. In addition to offering Flemish companies and research centres financial support, advice and a network of potential partners in Flanders and abroad, it also supports the Flemish Government in defining and adapting its innovation policy.

The total funding of the IWT amounted to € 297 million in 2008. The scope of existing funding programs is quite broad, including industrial R&D projects, EUREKA-projects, feasibility studies and innovation projects for SME’s, support to industrial networks (sectoral research, technological advisory services, innovation stimulation), support to universities for strategic basic research (SBO), support to higher education engineering schools for technology diffusion actions, individual grants for PhD and post-doc research, support to universities for exploitation of their R&D-results and to larger “ad hoc” initiatives as decided by the Flemish government.

In its competence of also coordinating regional innovation initiatives such as regional development agencies, technological advisory services, sectoral research centres and industrial federations, the IWT can be viewed as both, a program owner (in close co-operation with the Flemish Minister of Innovation) and a program manager (selection and follow-up of research and innovation projects).

2.2. Funding by the IWT

IWT’s funds for supporting R&D and innovation are directed to small as well as to large companies, universities, third level education institutions and other Flemish innovative organizations, individually or collectively. A wide range of activities is supported through this financial support, including feasibility studies, research and development projects for companies, strategic basic research and grants for research institutions and researchers, network projects and translation research for intermediary organizations.

Every year, some 600 companies benefit from IWT support (overall support, all measures cumulated). While in the past it was mainly manufacturing companies that have solicited the support of the IWT, nowadays, service providers are more and more represented.

In order to encourage smaller firms to perform R&D, a special program for SMEs has been put in place (the “KMO programma”). The maximum project cost a firm can submit under this program is € 200,000. Of these total project costs, the maximum subsidy rate is of 35% for a medium-sized company, and an extra 10% (hence 45% of the total project costs) for a small-sized company. If an SME collaborates with a public research institute or an international partner, it can submit a proposal of a maximum of € 250,000. If it collaborates with another firm (nationally), it can get 10% top-up in the subsidy rate.

Besides the KMO program, the IWT has the R&D program. In the latter, the basic subsidy rate that is of 15% for development and 40% for research. Furthermore, additional 10% are available for medium-sized enterprises and 20% additional support for small firms. Further support may be granted to projects that meet specific policy targets, like e.g. the promotion of sustainable technological development or cooperation with research institution. Finally, an extra 10% of support may be granted to projects involving substantial collaboration of several companies, provided that at least one is an SME or that the project entails an international cooperation. The general feature of the IWT subsidy scheme is its bottom-up character: it is a permanently open and non-thematic scheme.

With regards to the evaluation procedures, the IWT has a well-developed set of procedures for project evaluation, based on internal and external referees to evaluate the ex-ante effectiveness of the project proposals.

Initially, the evaluation criteria were heavily focussed on the scientific qualities and technological risks of the project. Gradually however, the economic dimension became equally – or even more – important, reflecting the shift from a purely R&D policy towards a more innovation related policy focus. This economic evaluation doesn’t only concern the financial feasibility of the project or the commercial prospects for the innovating firm but also the economic return ‘for Flanders’.

As part of the IWT’s evaluation, other ‘societal’ qualities of the project–mainly concerning environmental sustainable development – are also considered, though to a lesser extent than the economic criteria. The evaluation gives access to extra support in the form of a priority ranking across existing subsidy schemes and of a financial bonus of 10% on the project budget. Hence, project evaluation in Flanders is closely linked to general policy criteria in a bottom-up innovation policy design.

3. Theoretical premises and literature review

3.1. Theory

In economic literature, the impact of innovation policies – and particularly direct subsidies for R&D – on firms’ innovative behaviour has been of interest for many years now. The economic justification for governmental intervention for private sector R&D activities relies on the familiar market failure arguments (Arrow, 1962). Given these market failure arguments, most governments in industrialized economies attempt to correct them by designing policies, like for instance intellectual property right systems to improve appropriability of knowledge, tax reliefs to reduce the cost of R&D (see Hall and Van Reenen, 2000), direct subsidy programs

[1] The background information and stylized facts stem from Larosse (2004), http://www.eurotransbio.eu and www.iwt.be.

(see e.g. David et al., 2000, for a survey), public venture capital (see Hall and Lerner, 2010, for a survey) or loans with low interest rates. A detailed overview of the existing policy mix and its potential effects on R&D activities would however be beyond the scope of this study, since we are merely interested in direct subsidies.

According to Arrow (1962), the market failure arguments can be summarized into three main issues: (i) increasing returns, (ii) inappropriability of knowledge and (iii) uncertainty.

(i) Information is characterized by increasing returns to scale insofar that once the information is produced, it can be used multiple times, regardless of the scale of production. Since the same unit of information can be used multiple times by the same or by a different user, the cost of information production is not dependent on the scale on which the information is used (Arrow, 1962, 1996, 1999; Lamberton, 1996). There are generally relative high fixed costs of establishing an economic unit for the production of information and the marginal cost of providing a unit of information is far less than the average costs of information production. Indeed, from a welfare point of view, the information concerning an invention should be available completely free of charge, with the exception of the cost of transmitting the information (often very low or close to zero though). Even though this would ensure an optimal utilization of the information, it would present very little, if not no, incentive to invest in knowledge production.

(ii) In terms of inappropriability, it is a well-known fact that because of the non-rival and non-exclusive character of knowledge, a firm can never appropriate all the benefits of its R&D investments, even though it has to bear the entire costs. A part of the created knowledge always spills over to other agents, so that many agents can benefit from an investment done by the one firm. Hence, the incentive to be the investing agent is reduced due to this inappropriability of knowledge.

(iii) The third argument is linked to the uncertainty that is concurrent to innovative activities. As a matter of fact, innovation is not only uncertain in that one does not always know whether the desired result of the technological change or the innovation will be obtained in terms of output, but very often, one cannot be sure about the market success of an innovation either. Indeed, the path from a brilliant idea to a technical invention and eventually to a successful market application is long, risky and sinuous. In other words, the output of an invention or an innovation can never be perfectly predicted by its input (Arrow, 1962). Hence, in order to undertake such an uncertain project, a firm has to be willing to bear the inherent risk of this endeavour. Since the assumption is that firms are often risk-averse, this will lead to a sub-optimal allocation of risk, meaning that there will be discrimination against risky projects (Arrow and Lind, 1970). In line with the uncertainty (moral hazard) argument, firms often face financial constraints if they do not have sufficient internal resources to undertake an R&D project. Indeed, R&D investments are generally characterized by high firm specific investment and adjustment costs on the one hand, and low collateral value on the other hand. An important share of R&D investment consists in financing R&D employees and training, and hence, a large part of the investment is immediately sunk. Compared to investment in physical capital, R&D itself cannot be used as collateral in credit negotiations (see Hall (2002) for a comprehensive survey of financial constraints). Hence, R&D investments are often hampered by a lack of external lenders or investors. Finally, the market uncertainty for new products delays investment in R&D, reducing the total R&D in the economy. This is even more accurate for projects of more basic research, as the latter are further away from the market and its potential use may be largely unknown by the time of the investment (see e.g. Czarnitzki and Hottenrott, 2011; Czarnitzki et al., 2011). Recent literature (real options theory; see e.g. Dixit and Pindyck, 1994) emphasizes the irreversibility of investments. In other words, firms incur additional opportunity costs by turning down the option to wait for information, and thus by investing today, they eliminate their chance of investing at any time in the future. As a consequence of this uncertainty, investment will decrease. See Czarnitzki and Toole (2007, 2011) for empirical applications on R&D investment. They indeed find that investments fall as product market uncertainty increases.

While the third argument relates to uncertainty and financial market constraints, the two prior arguments, namely the inappropriability of R&D and the increasing returns are associated with positive spillovers and increased consumer surplus. In practice this means that it is socially desirable to subsidize an R&D project if it is associated to high social returns (and provided that the project in question would not have taken place if the firm would have been left on its own, as the project cost may not cover the expected private return). As a consequence of the above, it is a largely shared view that R&D activities are difficult to finance in a freely competitive market place. Support of this view in the form of economic-theory modelling dates back to Schumpeter (1942) and was further developed by Nelson (1959) and Arrow (1962).

3.2. Empirical evidence

The predominant question analyzed by empirical literature is whether public subsidies crowd-out private investment or whether they stimulate them. In a survey of the literature on the impact of public R&D subsidies on private R&D expenditure, David et al. (2000) find that most studies are subject to a potential selection bias. Indeed, since neither the fact of applying, nor the fact of receiving a public subsidy can be viewed as random, the selection into such a process has to be taken into account for the results of an analysis to be credible.

Hall and Maffioli (2008) conclude that in empirical literature since 2000 addressing selection bias explicitly total crowding out effects were only found for the US Small Business Innovation Research (SBIR) program: Wallsten (2000) employing a sample of 479 observations and using a 3SLS approach finds crowding out effects. However, he could not exclude the possibility that the grants might have had a positive effect on keeping the funded firms' R&D activities constant, which might not have been possible otherwise. The vast majority of other studies find positive results for R&D intensity or patent activity.

Other studies in this area include Almus and Czarnitzki (2003) finding that Eastern German firms which received public subsidies increased their innovation activities by about 4% points, Czarnitzki and Hussinger (2004) as well as Czarnitzki and Licht (2006) finding a positive impact in evaluating the effect of public R&D funding on R&D intensity and patent outcome in Germany, Hussinger (2008) confirming earlier results for Germany using semi-parametric selection models, Duguet (2004) focusing on growth of the ratio of firms' R&D to sales for France, Aerts and Schmidt (2008) reporting positive treatment effects on R&D for Germany and Flanders applying a variant of the conditional difference-in-difference estimator for repeated cross-sections, Czarnitzki and Lopes-Bento (2011a) using cross-country harmonized survey data, and Czarnitzki and Lopes-Bento (2011b) in a multiple treatment effect analysis for German firms. All these studies reject total crowding out, even though some find evidence of partial crowding out. Takalo et al. (2012) model the application and R&D investment decisions of the firm and the subsidy granting decision of the public agency in charge of the program to estimate the expected welfare effects of targeted R&D

subsidies using R&D project level data from Finland. They find that the social rate of return on targeted subsidies is 30–50%, but that spillover effects of subsidies are smaller than effects on firm profits. González et al. (2005) use IV models to evaluate R&D policy programs on about 2000 Spanish firms. The authors find that private R&D investment is stimulated by subsidies in Spain. Busom (2000) applying a parametric selection model finds that public R&D subsidies stimulate private R&D spending positively on average, but that partial crowding out cannot be rejected. For a more comprehensive survey of most recent studies, see Cerulli (2010).

4. Econometric method

Modern econometric techniques addressing selection bias have been studied for many years now (see Heckman et al., 1999; Imbens and Wooldridge, 2009, for surveys). Different estimation strategies include the (conditional) difference-in-difference estimator, control function approaches (selection models), instrumental variable (IV) estimation, non-parametric (matching) techniques based on propensity scores and others such as regression discontinuity designs.

The difference-in-difference method requires panel data with observations before and after (or while) the treatment (change of subsidy status). As our database (to be described in the following subsection) consists of three cross-sections and not of a panel (73% of the firms are observed only once), we cannot apply this estimator.

For the application of an IV estimator or a selection model, one needs a valid instrument (or an "exclusion restriction" in the selection model case) for the treatment variables. As finding valid instruments (or exclusion restriction) turns out to be very challenging in the present context, we primarily apply matching estimators in this study. Matching has the advantage to require no assumptions about functional forms and error term distributions. The downside, however, is that it only controls for the selection on observables. Hence, we have to maintain the assumption that we observe all important determinants driving the selection into program participation, namely the receipt of an IWT subsidy.[2] As this is a clear limitation, we also report our endeavor on how to find instrumental variables and present some robustness checks concerning our main findings using IV regressions at the end of the empirical results section.

Our fundamental evaluation question can be illustrated by an equation describing the average treatment effect on the treated firms, respectively:

$$E(\alpha_{\mathrm{TT}}) = E(Y^{\mathrm{T}}|S=1) - E(Y^{\mathrm{C}}|S=1) \quad (1)$$

where Y^{T} is the outcome variable. The status S refers to the group: $S=1$ is the treatment group and $S=0$ the non-treated firms. Y^{C} is the potential outcome which would have been realized if the treatment group ($S=1$) had not been treated. As previously explained, while $E(Y^{\mathrm{T}}|S=1)$ is directly observable, it is not the case for the counterpart. $E(Y^{\mathrm{C}}|S=1)$ has to be estimated. Because of a potential selection bias due to the fact that the receipt of a subsidy is not randomly assigned, $E(Y^{\mathrm{C}}|S=1) \neq E(Y^{\mathrm{C}}|S=0)$ and the counterfactual situation cannot simply be estimated as average outcome of the non-participants. Rubin (1977) introduced the conditional independence assumption (CIA) to overcome this selection problem, that is, participation and potential outcome are statistically independent for firms with the same set of exogenous characteristics X. In the case of matching, this potential "untreated outcome" of treated firms is constructed from a control group of firms that did not receive subsidies. The matching relies on the intuitively attractive idea to balance the sample of program participants and comparable non-participants. Remaining differences in the outcome variable between both groups are then attributed to the treatment. If the CIA holds, it follows that

$$E(Y^{\mathrm{C}}|S=1, X) = E(Y^{\mathrm{C}}|S=0, X) \quad (2)$$

and the average treatment effect on the treated can be written as:

$$E(\alpha_{\mathrm{TT}}) = E(Y^{\mathrm{T}}|S=1, X=x) - E(Y^{\mathrm{C}}|S=0, X=x) \quad (3)$$

In the present analysis, we conduct a variant of the nearest neighbour propensity score matching, namely caliper matching. More precisely, we pair each subsidy recipient with the single closest non-recipient. The pairs are chosen based on the similarity in the estimated probability of receiving such a subsidy, meaning the propensity score stemming from a probit estimation on the dummy indicating the receipt of subsidies S. Matching on the propensity score has the advantage not to run into the "curse of dimensionality" since we use only one single index as matching argument (see Rosenbaum and Rubin, 1983). In addition of matching on the propensity score, we also require the observations of firms in the selected control group to belong to the same year and to have a similar patent stock as the firms in the treatment group.

Furthermore, it is essential that there is enough overlap between the control and the treated group (common support). In practice, the samples of treated and controls are frequently restricted to common support. We thus calculate the minimum and the maximum of the propensity scores of the potential control group, and delete observations on treated firms with probabilities larger than the maximum and smaller than the minimum in the potential control group.

In order to avoid "bad matches", we impose a threshold (a "caliper") to the maximum distance allowed between the treated and the control unit. If the distance is above this pre-defined threshold, the treated observation is dropped from the sample to avoid bias in the estimation (see also Smith and Todd, 2005). The detail of our matching protocol is summarized in Table 1.

5. Data and variables

The data used in this paper stem from the Community Innovation Survey (CIS) of Flanders.[3] More precisely, they stem from the CIS4, covering the years 2002–2004, the CIS5, covering 2004–2006 and CIS6, covering 2006–2008. Furthermore, the data has been complemented by data from the Belfirst dataset and has been merged with information from the ICAROS dataset of the IWT. The latter provides detailed information on the amounts of the grants as well as on grants received in previous periods and the duration of the funded projects.

Our sample concerns innovative as well as non-innovative firms and covers manufacturing as well as business related services sectors.[4] In total, the sample consists of 4,761 observations, out of which 1,948 are innovative firms and 292 received a public R&D subsidy from the Flemish government. Tables A.1 and A.2 in

[2] Matching estimators have been applied and discussed by many scholars, amongst which Angrist (1998), Dehejia and Wahba (1999), Heckman et al. (1997, 1998a, 1998b), Lechner (1999, 2000) and Smith and Todd (2005).

[3] The CIS covers all of the EU member states, Norway and Iceland using a largely harmonized questionnaire throughout participating countries. The CIS databases contain information on a cross-section of firms active in the manufacturing sector and in selected business services.

[4] According to the 3rd edition of the Oslo Manual – which is the definition followed by the CIS – an innovative firm is one that has implemented an innovation during the period under review. An innovation is defined as the implementation of a new or significantly improved product (good or service) or process or service (see Eurostat and OECD, 2005).

Table 1
The matching protocol.

Step 1	Specify and estimate a probit model to obtain the propensity score $\hat{P}(X)$
Step 2	Restrict the sample to common support: delete all observations on treated firms with probabilities larger than the maximum and smaller than the minimum in the potential control group. (This step is also performed for other covariates that are possibly used in addition to the propensity score as matching arguments)
Step 3	Choose one observation from the subsample of treated firms and delete it from that pool
Step 4	Calculate the Mahalanobis distance between this firm and all non-subsidized firms in order to find the most similar control observation. $\mathrm{MD}_{ij} = (Z_j - Z_i)' \Omega^{-1}(Z_j - Z_i)$ where Ω is the empirical covariance matrix of the matching arguments based on the sample of potential controls. We use caliper matching, first introduced by Cochran and Rubin (1973). The intuition of caliper matching is to avoid "bad" matches (those for which the value of the matching argument Z_j is far from Z_i) by imposing a threshold of the maximum distance allowed between the treated and the control group. That is, a match for firm i is only chosen if $\|Z_j - Z_i\| < \varepsilon$, where ε is a pre-specified tolerance
Step 5	Select the observation with the minimum distance from the remaining control group. (Do not remove the selected controls from the pool of potential controls, so that it can be used again.) If the control group is empty after applying the caliper threshold, the treated firm is dropped from the sample and is not taken into account in the evaluation
Step 6	Repeat steps 3–5 for all observations on subsidized firms
Step 7	Using the matched comparison group, the average effect on the treated can thus be calculated as the mean difference of the matched samples: $\widehat{\alpha}_{\mathrm{TT}} = 1/n^{\mathrm{T}}\left(\sum_i Y_i^{\mathrm{T}} - \sum_i \widehat{Y}_i^{\mathrm{C}}\right)$ with $\widehat{Y}_i^{\mathrm{C}}$ being the counterfactual for i and n^{T} is the sample size (of treated firms)
Step 8	As we perform sampling with replacement to estimate the counterfactual situation, an ordinary t-statistic on mean differences is biased, because it does not take the appearance of repeated observations into account. Therefore, we have to correct the standard errors in order to draw conclusions on statistical inference. We follow Lechner (2001) and calculate his estimator for an asymptotic approximation of the standard errors

Appendix A show the industry structure as well as the firm size distribution of our sample.

The receipt of a subsidy form the IWT is denoted by a dummy variable equal to one for firms that received public R&D funding and zero otherwise.

5.1. Outcome variables

As outcome variables, we consider the internal R&D investment, *RDINT*, being the ratio of internal R&D expenditures[5] to sales (multiplied by 100) as well as R&D employment, *RDEMP*, being the ratio of R&D employment to total employment (multiplied by 100).

5.2. Control variables

We use several control variables in our analysis likely to impact the fact of whether or not a firm applies and receives public support for its R&D activities. The number of employees (*EMP*) takes into account possible size effects. As the firm size distribution is skewed, the variable enters in logarithms (ln *EMP*). We also allow for a potential non-linear relationship by including $(\ln EMP)^2$.

In addition, we include a dummy variable capturing whether or not a firm is part of a group (*GP*), and if so, whether it has its headquarters on foreign territory (*FOREIGN*). Firms that belong to a group may have a lower incentive to apply for subsidies as small firms that have a large majority shareholder do not qualify for the SME program where larger subsidy rates are granted. In contrast, however, firms belonging to a group may be preferred by the funding agency as the group membership possibly promises knowledge spillovers and thus economies of scope from the R&D process more likely than for stand-alone companies. In addition, firms belonging to a network of firms may benefit from better communication structures and thus are better informed about possible funding sources including public technology policy programs. Subsidiaries having a foreign parent, however, may be less likely to receive subsidies, as the parent may prefer to apply in its home country or because the funding agency gives preference to local firms. Furthermore, foreign parents having Flemish subsidiaries are typically large multinational companies and thus the local subsidiary does not qualify for special SME-support which could reduce its likelihood to apply.

The log of the firm's age (ln *AGE*) is included in the analysis as it is often claimed that older firms are more reluctant to pursue innovation, and would thus be less likely to apply, all else constant.

Previous experience in successful R&D activities plays a vital role when applying for public support, as governments often adopt a picking-the-winner strategy and hence might favor firms with previous success stories. Therefore, we include the patent stock (*PS*) in our regression. The patent stock enters into the regression as patent stock per employee to avoid potential multicollinearity with firm size (*PS*/*EMP*). Even though "not all inventions are patentable" and "not all inventions are patented" (Griliches, 1990, p. 10), the patent stock is the best approximation we have for past (successful) innovation activities as data on previous R&D expenditures are not available. The patent stock information stem from the EPO database and are computed as a time series of patent applications with a 15% rate of obsolescence of knowledge capital, as is common in the literature (see e.g. Jaffe, 1986; Hall, 1990; Griliches and Mairesse, 1984): $\mathrm{PS}_{i,t} = \mathrm{PS}_{i,t-1} \times 0.85 + \text{patent applications}_{i,t}$.

We also control for the degree of international competition by including an export-to-sales ratio in our analysis (*EXPORT*). Firms that engage more heavily in foreign markets may be more innovative than others and, hence, more likely to apply for subsidies.

We further include the labor productivity as a covariate, measured as sales per employee, *LAB_PRO*, since high labor productivity may be a relevant determinant for receiving public funds if the government follows a picking-the-winner strategy rigorously.

Furthermore, we also control for publicly supported R&D projects in the past. We include a variable equal to the number of IWT projects a firm has completed within the three preceding years (*IWT_PAST3YRS*). Last but not least, industry dummies control for unobserved heterogeneity across sectors and time dummies capture macroeconomic shocks.

5.3. Timing of variables

As mentioned above, each wave of the survey covers a three-year period. In order to avoid endogeneity between the dependent variables and the covariates to the largest extent, we employ lagged values wherever possible. For instance, suppose the dependent variables are measured in period t. Then *EMP*, *PS*/*EMP*, *LAB_PRO* and *EXPORT* are measured at the beginning of the survey period, i.e. in $t-2$.

The information on *GP* and *FOREIGN* is only available such that the question covers the whole 3-year period, i.e. $t-2$ to t. For instance, "Did your firm belong to a group during the period 2004–2006?" We consider *AGE* as truly exogenous and hence it is measured in period t.

[5] The CIS definition of R&D expenditure follows the Frascati Manual (OECD, 1993).

Table 2
Descriptive statistics.

Variables	Unsubsidized firms, $N = 4469$		Subsidized firms, $N = 292$		Results of t-tests on mean differences
	Mean	Standard deviation	Mean	Standard deviation	
Covariates					
IWT_PAST3YRS	0.023	0.181	0.736	2.436	***
*PS/EMPL**1000*	1.340	8.254	13.456	27.277	***
ln *EMP*	3.484	1.298	4.690	1.884	***
FOREIGN	0.245	0.430	0.284	0.452	
EXPORT	0.376	0.484	0.545	0.499	***
GP	0.466	0.499	0.664	0.473	***
ln *AGE*	3.113	0.777	3.153	0.889	
ln *LAB_PRO*	5.197	0.867	5.279	0.701	*
Year 2006	0.257	0.437	0.349	0.478	***
Year 2008	0.426	0.494	0.257	0.438	***
Outcome variables					
RDINT	0.894	5.322	7.579	12.694	***
RDEMP	2.646	9.960	18.287	21.980	***

Note: *** (**, *) indicate a significance level of 1% (5%, 10%).

5.4. Descriptive statistics

Table 2 shows the descriptive statistics of the variables of our sample. As we can see, almost all means of the variables are significantly different between the treated and the non-treated firms.

For instance, firms receiving subsidies from the IWT are on average larger, more export oriented, belong to a group more often and have a higher labor productivity. Furthermore, they have had significantly more funded projects in the last three years and have a higher patent stock per employee. With regards to the outcome variables, funded firms have on average higher R&D intensity and have more R&D employees. The econometric analysis in the next section will reveal to which extent these differences can be attributed to the treatment.

6. Econometric results

In order to apply the matching estimator as presented in the previous section, we first have to estimate a probit model to obtain the predicted probability of receiving an IWT grant. We can see in Table 3 that with the exception of the coefficients of the group, the age and the labor productivity variables, all the other coefficients are significantly different from zero and hence are important in driving the selection of into the funding scheme.

Table 3
Probit estimation.

	Coefficient	Standard errors
IWT_PAST3YRS	0.755***	0.084
*PS/EMPL**1000*	0.016***	0.002
ln *EMP*	−0.140	0.105
(ln *EMP*)^2	0.042***	0.011
FOREIGN	−0.429***	0.099
EXPORT	0.655***	0.103
GP	0.098	0.091
ln *AGE*	−0.073	0.047
ln *LAB_PRO*	0.008	0.052
Intercept	−1.750***	0.407
Test on joint significance of industry dummies		$\chi^2(9) = 94.29$***
Test on joint significance of time dummies		$\chi^2(2) = 24.61$***
McFadden R^2		0.308
Number of observations		4761

Note: *** (**, *) indicate a significance level of 1% (5%, 10%).

As explained in the previous section, a necessary condition for the validity of the matching estimator is common support. In our case, 14 treated observations are lost because no common support could be found. In addition, 26 treated observations are dropped because of the caliper we impose on the maximum distance between neighbors. Hence, a total of 40 observations are deleted from the sample of treated firms.

As shown by Table 4, all our covariates are well balanced after the matching. Hence, we can conclude that our matching was successful and that we found a neighbor for each treated firm. The only variables where there still is a significant difference after the matching are our outcome variables. This difference can be attributed to the subsidy. The treatment effect in terms of R&D intensity amounts to 3.73% points and in terms of R&D employment intensity to 9.57% points. As a consequence, we can reject the null hypothesis of total crowding out effects. In line with previous findings on the effect of direct subsidies on firms in Belgium and other European countries, we can conclude that the IWT grants trigger investment into R&D in the recipient firms.

In the subsequent subsections, we use this baseline result in order to explore further policy relevant questions concerning program evaluation.

6.1. Stability of treatment effects over time

In times of a changing macroeconomic environment, it is useful for a policy maker to know whether the effect of a policy varies over time. As we use data from 2004, 2006 and 2008, it is interesting to test whether the treatment effects we observe are stable over this period. In other words, we want to see whether the effect of a subsidy receipt changes between periods t, $t+1$ and $t+2$. In order to do so, we regress the treatment effect, α_i, on the time dummies Y2006 and Y2008 (so that 2004 is the base year in this regression). If the policy effects are stable, we expect that none of the time dummies is individually significant and that the whole regression model is insignificant (all coefficients are jointly zero).

As shown by the results of Table 5, for both outcome variables under review, we do not find evidence that the treatment effect varies over time. The coefficients are all jointly insignificant. We can thus conclude that the effects of IWT subsidies on R&D employment and internal R&D investment remain stable over time and that there is no evidence for a decline of the effect of the policy measure under consideration over time. It would be interesting to repeat this exercise with data covering later years, i.e. times when the financial crisis became effective.

Table 4
Matching results on the full sample.

Variables	Selected control group, $N = 252$		Subsidized firms, $N = 252$		p-Value of t-tests on mean difference
	Mean	Standard deviation	Mean	Standard deviation	
Covariates					
IWT_PAST3YRS	0.258	0.709	0.349	0.821	$p = 0.222$
*PS/EMPL*1000*	7.932	20.092	8.354	20.032	$p = 0.831$
ln *EMP*	4.302	1.876	4.359	1.678	$p = 0.745$
(ln *EMP*)^2	22.011	17.906	21.808	15.586	$p = 0.904$
FOREIGN	0.262	0.441	0.262	0.441	$p = 1.000$
EXPO	0.599	0.491	0.560	0.497	$p = 0.415$
GP	0.544	0.499	0.623	0.486	$p = 0.103$
ln *AGE*	3.099	0.873	3.085	0.860	$p = 0.866$
ln *LAB_PRO*	5.291	0.792	5.252	0.701	$p = 0.606$
Outcome variables					
RDINT	3.151	10.373	6.883	12.140	$p = 0.001$
RDEMP	8.061	18.254	17.627	21.603	$p < 0.001$

Table 5
OLS regression testing for stability of treatment effect over time.

	Dependent variable: α_i					
	R&D employment			R&D intensity		
	Coefficient	Standard errors	$p > \|t\|$	Coefficient	Standard errors	$p > \|t\|$
Y2006	5.299	3.985	0.185	0.905	1.824	0.620
Y2008	1.513	4.578	0.741	−0.017	2.988	0.995
Intercept	7.317	3.285	0.027	3.424	1.619	0.036
Number of observations		252			252	
Overall significance		$F(2, 208) = 0.16$			$F(2, 208) = 1.02$	

Note: Standard errors are clustered at the firm level, as a few firms appear more than once in the database. *** (**, *) indicate a significance level of 1% (5%, 10%).

6.2. The effect of multiple subsidized projects at the same time

Since it is possible for one firm to benefit from more than one subsidized project during the same period, we test whether the treatment effects are larger for firms that have more than one subsidized project compared to firms that have only one subsidized project or whether this effect is declining with the number of projects. On average, each treated firm in our sample has 1.5 projects in a given year. Out of our 292 treated firms in the sample, 66% have only one project in a given year, 19% have two projects. One firm had 26 projects. Therefore, it is interesting to test whether the estimated treatment effect increases with the number of projects a firm has or whether we witness a decrease in the effect, indicating that supporting several projects in a same company would not be an efficient policy to pursue. In order to test this, we regress the estimated treatment effect, α_i, on the number of projects a firm has (see Table 6). We also considered non-linear specifications, as the concern would be that decreasing returns exist, that is, if more projects are granted at the same time, partial crowding out effects could emerge. We implemented the potentially non-linear specification by (a) including a squared and cubic term in the regression as shown in Table 6; (b) estimating a non-parametric kernel regression (Nadaraya Watson estimator). Both of these robustness checks confirm our linear specification as shown in Table 6. In the parametric OLS regression, the squared and cubic terms were jointly insignificant, and the non-parametric regression did yield a regression line very similar to the linear prediction obtained with the OLS regression below. We also performed a *J*-test (Davidson and MacKinnon, 1981) on whether the non-parametric regression is to be preferred, and the parametric linear regression was not rejected.

6.3. Evaluation of the treatment effects of "consecutive clients"

Funding agencies often consider previous experience (with R&D projects as well as with specific funding schemes) an important determinant in the selection process into a public subsidy scheme. As a consequence, some firms receive subsidies on a regular basis. It is thus interesting to evaluate whether this is a justified approach or whether one witnesses a decline of the subsidy effect over time if the same firm is a repetitive beneficiary. We thus test whether we observe a decline of the treatment effect if a firm gets funding repeatedly or whether the effects remain stable.

In order to test this, we regress the treatment effects, α_i, of our respective outcome variables after the matching on a dummy variable indicating whether the firm had an IWT project that ended in the previous 3 years. As shown by Table 7, we do not find evidence that the treatment effect is smaller for firms that have

Table 6
Regression on the treatment effect on the number of supported projects.

	Dependent variable: α_i					
	R&D employment			R&D intensity		
	Coefficient	Standard errors	$p > \|t\|$	Coefficient	Standard errors	$p > \|t\|$
Number of IWT projects	2.321	0.521	0.000	0.916	0.306	0.003
Intercept	5.789	2.122	0.007	2.240	1.141	0.051
Number of observations		252			252	
Overall significance		$F(1,208) = 19.83^{***}$			$F(1, 208) = 8.96^{***}$	

Note: Standard errors are clustered at the firm level, as a few firms appear more than once in the database. *** (**, *) indicate a significance level of 1% (5%, 10%).

Table 7
Regression testing the treatment effect of "consecutive clients".

	Dependent variable: α_i					
	R&D employment			R&D intensity		
	Coefficient	Standard errors	$p > \|t\|$	Coefficient	Standard errors	$p > \|t\|$
Dummy (IWT subsidy in the last 3 years)	−5.043	7.768	0.517	4.581	3.503	0.192
Intercept	9.927	1.814	0.000	3.151	1.054	0.001
Number of observations	252			252		
Overall significance	$F(1, 208) = 0.42$			$F(1, 208) = 1.71$		

Note: Standard errors are clustered at the firm level, as a few firms appear more than once in the database. *** (**, *) indicate a significance level of 1% (5%, 10%).

Table 8
Matching results, full sample: controlling for other subsidies.

Variables	Selected control group, $N = 215$		Subsidized firms, $N = 215$		p-value of t-tests on mean differences
	Mean	Standard deviation	Mean	Standard deviation	
Covariates					
IWT_PAST3YRS	0.233	0.613	0.298	0.680	$p = 0.350$
*PS/EMPL*1000*	5.628	17.357	6.191	17.597	$p = 0.767$
ln *EMP*	4.482	1.927	4.318	1.656	$p = 0.337$
$(\ln EMP)^2$	23.794	18.958	21.376	15.194	$p = 0.174$
FOREIGN	0.293	0.456	0.247	0.432	$p = 0.339$
EXPO	0.553	0.498	0.540	0.500	$p = 0.932$
GP	0.619	0.487	0.623	0.486	$p = 0.930$
ln *AGE*	3.148	0.851	3.075	0.852	$p = 0.465$
ln *LAB_PRO*	5.331	0.739	5.230	0.701	$p = 0.387$
Outcome variables					
RDINT	3.864	10.655	6.780	12.171	$p = 0.015$
RDEMP	9.633	18.772	17.635	21.746	$p < 0.001$

received support for their R&D projects repeatedly. In other words, we do not find that the effect of a subsidy receipt decreases if a firm had completed an IWT project in the three years preceding the receipt of a new grant.

6.4. Taking subsidies from other sources into account

Since a firm that receives support from the Flemish government can also ask for, and receive, support from other public agencies (i.e. form national or European entities), it is interesting to know for a policy maker if, and to which extent receiving a subsidy from another source influences the effect of a grant from a specific program under review (here: IWT grants). To test this, we re-estimate our matching routine, but instead of only matching on the propensity score, we additionally match on a dummy variable indicating whether a firm received a subsidy from an agency other than the IWT (for instance, the federal government or the EU). This means that an IWT recipient that has also received EU funding would only be matched with a firm that did not get an IWT subsidy, but received support from the EU. As displayed by Table 8, we find that our initial results hold. All the covariates are well balanced after the matching, with the exception of the outcome variables where a significantly positive result remains.

In order to investigate whether firms also getting subsidies from other sources than the IWT show higher crowding out effects, we regress the estimated treatment effects on the dummy indicating the subsidy receipt from other entities. If other subsidies would lead to crowding out effects we would expect a negative and significant coefficient of the dummy variable. As Table 9 shows, however, the estimated coefficient is not significantly different from zero and thus we do not find evidence that a subsidy mix leads to crowding out effects.

Table 10
Probit estimation, only for innovative firms.

	Coefficient	Standard errors
IWT_PAST3YRS	0.648***	0.084
*PS/EMPL*1000*	0.014***	0.002
ln *EMP*	−0.158	0.118
$(\ln EMP)^2$	0.038***	0.012
FOREIGN	−0.423***	0.109
EXPO	0.442***	0.128
GP	0.072	0.103
ln *AGE*	−0.070	0.052
ln *LAB_PRO*	0.006	0.063
Intercept	−1.339***	0.488
Test on joint significance of industry dummies	χ^2 (9) = 64.04***	
Test on joint significance of time dummies	χ^2 (2) = 15.16***	
Number of observations	1,948	

Note: Standard errors are clustered at the firm level, as a few firms appear more than once in the database. *** (**, *) indicate a significance level of 1% (5%, 10%).

Table 9
Regression of treatment effect on the receipt of other subsidies (215 observations).

	R&D employment			R&D intensity		
	Coefficient	Standard errors	$p > \|t\|$	Coefficient	Standard errors	$p > \|t\|$
Dummy (subsidies received from other entities than the IWT)	−3.564	4.101	0.386	−0.034	2.161	0.987
Intercept	9.460	2.207	0.000	2.929	1.331	0.029
Number of observations		215			215	
Overall significance		$F(1, 183) = 0.76$			$F(1, 183) = 0.01$	

Note: Standard errors are clustered at the firm level, as a few firms appear more than once in the database.

Table 11
Matching results, of innovative firms only.

Variables	Selected control group, $N=262$		Subsidized firms, $N=262$		p-Value on the t-test on mean difference
	Mean	Standard deviations	Mean	Standard deviations	
Covariates					
IWT_PAST3YRS	0.305	0.726	0.321	0.766	$p=0.839$
*PS_EMPL*1000*	10.053	23.261	10.500	23.534	$p=0.851$
ln *EMP*	4.387	1.648	4.426	1.707	$p=0.818$
(ln *EMP*)^2	21.949	15.291	22.490	16.121	$p=0.733$
FOREIGN	0.271	0.445	0.263	0.441	$p=0.866$
EXPORT	0.595	0.492	0.573	0.496	$p=0.648$
GP	0.595	0.492	0.634	0.483	$p=0.442$
ln *AGE*	3.149	0.855	3.091	0.860	$p=0.507$
ln *LAB_PRO*	5.164	0.714	5.260	0.708	$p=0.188$
Outcome variables					
RDINT	3.235	8.328	6.745	11.622	$p<0.001$
RDEMP	8.145	13.390	17.220	20.670	$p<0.001$

Table 12
Matching results, KMO recipients only.

Variables	Selected control group, $N=112$		Subsidized firms, $N=112$		p-Value on the t-test on mean difference
	Mean	Standard deviations	Mean	Standard deviations	
Covariates					
KMO_PAST3YRS	0.098	0.354	0.152	0.429	$p=0.327$
*PS/EMPL*1000*	5.672	16.376	5.873	16.980	$p=0.536$
ln *EMP*	3.355	1.093	3.282	1.027	$p=0.626$
(ln *EMP*)^2	12.439	6.815	11.816	6.379	$p=0.502$
FOREIGN	0.045	0.207	0.036	0.186	$p=0.748$
EXPORT	0.616	0.488	0.553	0.499	$p=0.366$
GP	0.429	0.497	0.330	0.472	$p=0.149$
ln *AGE*	2.884	0.771	2.995	0.797	$p=0.311$
ln *LAB_PRO*	5.123	0.683	4.999	0.617	$p=0.178$
Outcome variables					
RDINT	2.583	7.323	7.088	12.780	$p=0.002$
RDEMP	8.533	20.722	18.726	22.678	$p=0.001$

The conclusions on the other tests that we performed (the stability of the treatment effects over time, the potential crowding out effects of having multiple projects as well as the concern that repeated funding of the same firm) when the other subsidies were not taken into account also hold in this version of the estimation. The tables displaying the details of these results can be found in Appendix B.

6.5. Using only innovative firms

In the previous estimations, we allowed that non-innovating firms are in the pool of potential controls for the matching routine. This is based on the idea that, for instance, certain small firms may rely heavily on IWT subsidies. In other words, it could be the case that a small firm could stop its R&D activities entirely if it would not be subsidized (see e.g. Czarnitzki, 2006). Now, however, we drop the non-innovators from the control group. We thus assume that the firms would stay innovative even if they did not get a subsidy. Although this might underestimate the treatment effect to a certain extent, it is an interesting robustness check. It allows us to see if the magnitude of the treatment effect depends to a large extent on the "zero observations", that is, non-innovators in the control group (see Appendix C for descriptive statistics of this sample).

Before re-estimating the treatment effects, we re-estimate the probit model on subsidy receipt for our new sample (see Table 10). We see that the same covariates are significant as in the total sample.

As shown by Table 11, the estimated treatment effects are very similar to the ones reported above. This reaffirms our model specification as apparently the nearest neighbors that are drawn when using the full sample for the control group (i.e. including non-innovators) are typically innovators.

6.6. Evaluating the KMO recipients separately

Between 2004 and 2010, about two thirds of all IWT grants were handed out under the label of the "KMO programma", a subsidy scheme designed for small and medium-sized enterprises. Given that stark presence of a single scheme within the landscape of programs, we performed the estimations reported above separately for these subsidy recipients. Since this program was designed in the goal of enhancing R&D and innovation in small and medium-sized firms, a firm has to fall into this category in order to be eligible for the KMO grants. Firms that do not fall into this category have, as a consequence, been deleted from the sample for this analysis.[6] For details on descriptive statistics for this sample, see Appendix C.

The results of the Probit model on the subsidy receipt are very similar to the full sample. Therefore, we omit a detailed presentation here. As shown by Table 12, the estimated treatment effects are in line with those reported when considering all IWT schemes. After the matching, all the covariates are well balanced with the exception of the outcome variables, where positive significant differences exist due to the receipt of a grant.

We thus confirm our earlier findings of input additionality in the KMO program. Furthermore, the treatment effects are somewhat larger in the case of KMO subsidies than in the full sample using

[6] According to the EU's definition, an SME should have less than 250 employees and has either sales less than €50 million or a balance sheet total less than €27 million. 800 firms are concerned by this eligibility restriction in our sample.

Table 13
Instrumental variable regressions using full sample and subsample of innovators.

Variables	IV regression, full sample (4,761 observations)				IV regression, innovator sample (1,948 observations)			
	R&D INTENSITY		*R&D EMPLOYMENT*		*R&D INTENSITY*		*R&D EMPLOYMENT*	
	Model 1		Model 2		Model 3		Model 4	
# of current IWT projects	1.242	***	2.701	***	1.074	***	2.342	***
	(0.369)		(0.629)		(0.314)		(0.508)	
*PS/(EMPL*1000)*	107.434	***	203.829	***	113.718	***	202.138	***
	(25.848)		(41.061)		(27.862)		(41.875)	
ln *EMP*	0.407		−1.104		0.162		−4.152	**
	(0.381)		(1.058)		(0.746)		(1.657)	
(ln *EMP*)^2	−0.064		0.044		−0.063		0.24	
	(0.049)		(0.120)		(0.082)		(0.168)	
FOREIGN	0.469		0.103		1.298	*	1.152	
	(0.374)		(0.557)		(0.720)		(1.018)	
EXPORT	0.827	***	2.71	***	1.218	**	4.073	***
	(0.197)		(0.378)		(0.587)		(0.966)	
GP	0.516	**	0.784	*	1.211	**	1.091	
	(0.250)		(0.414)		(0.538)		(0.856)	
ln *AGE*	−0.2	**	−0.533	**	−0.22		−0.636	
	(0.095)		(0.209)		(0.193)		(0.417)	
ln *LAB_PRO*	−0.446	***	0.109		−1.155	***	0.499	
	(0.133)		(0.185)		(0.382)		(0.501)	
Intercept	2.834	***	7.419	***	7.447	***	16.171	***
	(0.971)		(2.490)		(2.653)		(5.130)	
F test of excluded instruments	32.92	***	32.92	***	33.17	***	33.17	***
Hansen *J* over identification test	2.14		1.486		2.424		1.921	

Notes: *** (**, *) indicate a significance level of 1% (5%, 10%). Standard errors in parentheses are clustered. All models contain full sets of industry and time dummies (not reported). The number of current IWT projects has been instrumented with the number of IWT projects that ended in period $t-2$ as well as the average size (in terms of thsd. EUR) per project.

also other schemes as treatment. This finding was expected, since one would suppose that the effect of a grant is higher for an SME than for a larger firm in terms of its relative impact on total R&D efforts of the firm. Furthermore, the KMO subsidies cover 10% more of the cost of an R&D project for small firms than the other grants. The results are thus in line with what one would except to observe.

6.7. Robustness test: Taking potential selection on unobservable characteristics into account

In this subsection, we describe our efforts to search for instrumental variables in order to also estimate models controlling for "selection on unobservables" that complement the application of matching estimators.

As described in the previous section, special funding rules exist for SMEs. Besides the fact that SMEs are eligible for a specific program (the KMO program), they receive a higher percentage rate of the total project costs covered depending on whether they are medium-sized enterprises (a maximum of 35% is covered) or small-sized enterprises (a maximum of 45% is covered)[7] provided that they are not part of a group. Hence, a potential instrument for our analysis would have been the inclusion of dummy variables equal to one if a firm qualifies as a small-sized (respectively a medium-sized enterprise) that is not part of a group. As a matter of fact, given the extra expenses covered for small firms in the KMO program, respectively for small and medium-sized firms in the R&D program, the eligible firms could have an additional incentive to submit a project to the IWT, given that for the same cost of applying, the potential gain for the latter is higher. Since the definition of small and medium sized is decided upon by the European Commission and since a firm would not choose the number of its employees in light of the eligibility criteria of a specific subsidy program, such a dummy would fulfil the condition of being (i) correlated with the endogenous dummy variable indicating the treatment and (ii) uncorrelated with the error term of the structural equation once the we control for the other covariates.

Unfortunately, neither one of these two dummy variable was significant in the first stage regression on the subsidy receipt. Thus we did not find that our preferred instruments based on exploiting discontinuities in the program rules to be relevant. Consequently, we can only go ahead with a "second best" solution. An option that our data offers is to use lags of the subsidy receipt as instrumental variables. Although not perfect, as one might be concerned whether lagged subsidies are truly exogenous to the system of equations (as subsidies might be serially correlated), they at least allow a rough robustness check to see whether our main results hold when we abandon the conditional independence assumption and allow for selection on unobservables. We experimented with different lag structures and found that using "the number of subsidized projects that ended in period $t-2$" along with their average size (="total amount of the subsidy in thsd. EUR" divided by the number of subsidized projects) fulfil the statistical requirements for instrumental variables. We found that these two instruments are relevant in the first stage on the number of current subsidized projects, and also pass the over-identification test (Hansen *J*-test). Thus we conclude that at least in terms of statistical requirements, the instruments can be considered as valid.

Due to space limitations we only show the detailed results of the baseline regressions investigating the relationship between R&D intensity (and R&D employment) and the number of subsidized projects the firm is currently conducting. As Table 13 shows our main results identified with the matching estimator also hold when using IV regressions. The effect of the number of subsidized projects is positive and significant. In terms of magnitude of the effect, we also find similar results to the matching procedure.

We also obtain similar results when we use a treatment dummy instead of the number of currently funded projects as regressor (not

[7] The definition of medium and small sized firms is the one by the EU, i.e. a firm qualifies as small-sized firm if it has less than 49 employees and less than € 10 million of turnover. It qualifies as medium-sized firm if it has between 50 and 249 employees and an annual turnover larger than € 10 million and smaller than € 50 million.

presented in detail). Furthermore, using parametric treatment (or "selection") models also result in the same conclusions.

7. Macroeconomic impact of IWT subsidies

In order to put the treatment effect into context, we conduct a 'back of the envelope' calculation on macroeconomic effects of the IWT subsidies.[8] In particular, we are interested in knowing how many (R&D) jobs these public grants create. In our sample, the R&D employment intensity amounts to 8% for the firms composing the selected control group (i.e. the treated firms, if they would not have gotten a subsidy).

At the sample median[9], total employment equals 49 employees, say 50 for easier calculation. Thus, in the counterfactual situation, a treated company would employ about 4 R&D employees (8% R&D employment intensity).

The treatment effect of a subsidy receipt concerning R&D employment intensity (i.e. the ratio of R&D employees to total employment) amounts to about 9.567% points, that is, each treatment in our context creates or secures about five R&D jobs.[10]

On average, the recipient firms have 1.5 IWT projects in a given year. In total, the IWT granted 3,019 projects with a total subsidy value of € 628 million between 2004 and 2010. As each company in our sample has on average 1.5 project in a given year, the estimated 5 R&D jobs are created 2,012 times in total (=3,019/1.5). Thus, in total the subsidies create 10,060 (R&D) jobs (2,012 * 5). Note that our treatment effects calculation is based on annual data. As the average project duration is about 20 months, the subsidies create a total sum of 16,800 (10,060 * (20/12)) person–years of R&D employment in the Flemish economy. Given that this result is achieved with a total budget of € 628 million of public budget, society pays about € 37,000 per created R&D employment per person per year.

From the Flemish R&D survey[11], we calculated the average cost of an R&D workplace. In other words, we calculated the R&D personnel cost, including the employers' social security contribution and the average annual working expenses for maintaining an R&D workplace. According to the R&D survey 2008, this cost amounts to € 64,000 on average. Thus, we can conclude that € 37,000 are sponsored by public funding, and the companies' average contribution amount to roughly € 27,000. Note that this is a back of the envelope calculation based on averages, and that we do not want to stress that this could be evidence on partial crowding out. As the subsidy rate is on average 40% of total project cost, one could expect that the industry share in the total work place cost should be higher than € 27,000. However, one has to keep in mind that we only take into account the firm's costs linked to R&D employment, disregarding other expenses like e.g. lab equipment and similar assets a firm has to invest in when conducting R&D projects. Such costs have not been taken into account in our ballpark figure.

8. Conclusion

The present paper presents new microeconometric evidence on the question of input additionality for a Belgian subsidy scheme. While many papers have analyzed the impact of subsidies on firms' innovative behavior, there is no econometric evidence on how this effect might change over time or how it might be affected if several projects are supported at the same time respectively if the firm gets support on a regular basis and when firms also receive support from other sources. This paper allows answering these questions, revealing important information for policy makers.

In line with the literature, we can reject the null hypothesis of total crowding out. Our further investigations allow us to conclude that we do not witness varying effects over time and that multiple grants at the same time do not cause crowding out effects. We can also conclude that there is no declining effect if firms get subsidies recurrently or if firms additionally receive support from other sources.

Finally, we estimate the macroeconomic impact of these Flemish subsidies on the local economy in terms of (R&D) jobs that are created. On average, one supported project creates 5 (R&D) jobs, out of which roughly € 37,000 are paid for by the public and € 27,000 by the firm. Keeping in mind that this figure is based only on the cost directly related to an employee and not including any equipment costs a firm encounters when hiring an additional person, our findings do not suggest crowding out. As we found that subsidies do increase the number of R&D employees, we can conclude that the additional investment in R&D does not just go into increased wages of the R&D personnel. In many previous studies, information on the number of employees allowing to draw this conclusion is often missing.

Appendix A. Industry structure and firm size distribution

See Tables A.1 and A.2.

Appendix B. Detailed results if the receipt of other grants is added as matching criterion

See Tables A.3–A.5.

Table A.1
Industry structure.

Industry	Number of firms
Food, beverages and tobacco	364
Textiles, clothing and leather	213
Chemicals (incl. pharma), rubber and plastics	346
Metal	400
Machinery and vehicles	415
Electronics, communication and instruments	193
Other manufacturing industries	1400
Trade	738
ICT services	415
Other business services	277
Total	4761

Table A.2
Size distribution.

Size class distribution	Number of firms
1–4	135
5–9	552
10–49	2,389
50–249	1,216
250–max.	469
Total	4761

[8] The following calculations refer to the first analysis, namely the entire sample (innovative and non-innovative firms), containing KMO and the R&D subsidy schemes.

[9] We take the median rather than the mean because of the skewness of the employment distribution.

[10] Average number of R&D employees in the selected control group: 50*0.08 = 4. If treatment is received, increase of 9.567 percentage points. Hence: 50*(0.08 + 0.096) = 8.8. Additional jobs created or secured: 9 – 4 = 5

[11] The Flemish part of the OECD R&D survey is conducted every second year by the Centre for R&D Monitoring at the KU Leuven.

Table A.3
OLS regression testing for stability of treatment effect over time.

	Dependent variable: α_i					
	R&D employment			R&D intensity		
	Coefficient	Standard errors	$p > \|t\|$	Coefficient	Standard errors	$p > \|t\|$
Y2006	−3.228	4.303	0.454	−1.579	1.735	0.364
Y2008	1.230	4.840	0.800	0.365	3.110	0.907
Intercept	8.653	3.368	0.011	3.302	1.431	0.022
Number of observations		215			215	
Overall significance		$F(1, 183) = 0.76$			$F(1, 183) = 0.54$	

Note: Standard errors are clustered at the firm level, as a few firms appear more than once in the database. *** (**, *) indicate a significance level of 1% (5%, 10%).

Table A.4
Regression of the treatment effect on the number of supported projects.

	Dependent variable: α_i					
	R&D employment			R&D intensity		
	Coefficient	Standard errors	$p > \|t\|$	Coefficient	Standard errors	$p > \|t\|$
Number of IWT projects	2.761	0.771	0.000	1.180	0.459	0.011
Intercept	3.622	2.438	0.139	1.044	1.278	0.415
Number of observations		215			215	
Overall significance		$F(1, 183) = 12.81^{***}$			$F(1, 183) = 6.61^{***}$	

Note: Standard errors are clustered at the firm level, as a few firms appear more than once in the database. *** (**, *) indicate a significance level of 1% (5%, 10%).

Table A.5
Regression testing the treatment effect of "consecutive clients".

	Dependent variable: α_i					
	R&D employment			R&D intensity		
	Coefficient	Standard errors	$p > \|t\|$	Coefficient	Standard errors	$p > \|t\|$
Dummy (IWT subsidy in the last 3 years)	13.911	7.999	0.084	10.563	5.020	0.037
Intercept	7.289	1.973	0.000	2.375	1.077	0.029
Number of observations		215			215	
Overall significance		$F(1, 183) = 3.02^{*}$			$F(1, 183) = 4.43^{**}$	

Note: Standard errors are clustered at the firm level, as a few firms appear more than once in the database. *** (**, *) indicate a significance level of 1% (5%, 10%).

Table A.6
Descriptive statistics, only innovative firms.

Variables	Unsubsidized firms, $N = 1656$		Subsidized firms, $N = 292$		Results on t-test on mean difference
	Mean	Standard deviations	Mean	Standard deviations	
Covariates					
IWT_PAST3YRS	0.054	0.281	0.736	2.436	***
*PS/EMPL*1000*	2.528	11.366	13.457	27.277	***
ln *EMP*	3.890	1.399	4.690	1.884	***
$(\ln EMP)^2$	17.089	12.137	25.535	19.005	***
FOREIGN	0.290	0.454	0.284	0.452	
EXPORT	0.434	0.496	0.545	0.499	***
GP	0.557	0.497	0.664	0.473	***
ln *AGE*	3.136	0.838	3.153	0.889	
ln *LAB_PRO*	5.284	0.791	5.279	0.701	
Outcome variables					
RDINT	2.413	8.533	7.579	12.694	***
RDEMP	6.851	14.916	18.287	21.980	***

Note: *** (**, *) indicate a significance level of 1% (5%, 10%).

Table A.7
Descriptive statistics, only small and medium sized firms.

Variables	Unsubsidized firms, N = 3839		Subsidized firms, N = 122		Results on t-test on mean difference
	Mean	Standard deviations	Mean	Standard deviations	
Covariates					
KMO_PAST3YRS	0.018	0.143	0.303	0.791	***
*PS/EMPL*1000*	1.249	8.182	8.178	20.797	***
ln *EMP*	3.133	0.981	3.296	1.034	*
(ln *EMP*)^2	10.775	6.359	11.923	6.570	*
FOREIGN	0.187	0.390	0.033	0.179	***
EXPO	0.367	0.482	0.525	0.501	***
GP	0.396	0.489	0.344	0.477	
ln *AGE*	3.069	0.765	2.996	0.786	
ln *LAB_PRO*	5.078	0.791	5.001	0.599	
Outcome variables					
RDINT	1.083	5.912	7.092	12.360	***
RDEMP	3.084	11.264	19.277	22.738	***

Note: *** (**, *) indicate a significance level of 1% (5%, 10%).

Appendix C. Descriptive statistics for the "innovative only" as well as the SME sample

See Tables A.6 and A.7.

References

Aerts, K., Czarnitzki, D., 2006. The Impact of Public R&D – Funding in Flanders, IWT Study No. 54, Brussels.
Aerts, K., Schmidt, T., 2008. Two for the price of one? Additionality effects of R&D subsidies: a comparison between Flanders and Germany. Research Policy 37, 806–822.
Almus, M., Czarnitzki, D., 2003. The effects of public R&D subsidies on firms' innovation activities: the case of Eastern Germany. Journal of Business and Economic Statistics 21 (2), 226–236.
Angrist, J.D., 1998. Estimating the labor market impact of voluntary military service using social security data. Econometrica 66, 249–288.
Arrow, K.J., 1962. Economic welfare and the allocation of resources for invention. In: Nelson, R.R. (Ed.), The Rate and Direction of Inventive Activity: Economic and Social Factors, National Bureau of Economic Research, Conference Series. Princeton University Press, Princeton, pp. 609–625.
Arrow, K.J., Lind, R.C., 1970. Uncertainty and the evaluation of public investment decisions. American Economic Review 60, 364–378.
Arrow, K.J., 1996. The economics of information: an exposition. Empirica 23 (2), 119–128.
Arrow, K.J., 1999. Information and the organization of industry. In: Chichilnisky, G. (Ed.), Markets, Information, and Uncertainty. Essays in Economic Theory in Honor of Kenneth J. Arrow. Cambridge University Press, Cambridge, pp. 19–25.
Busom, I., 2000. An empirical evaluation of the effects of R&D subsidies. Economics of Innovation and New Technology 9 (2), 111–148.
Cerulli, G., 2010. Modelling and measuring the effect of public subsidies on business R&D: a critical review of the economic literature. Economic Record 86, 421–449.
Cochran, W., Rubin, D., 1973. Controlling bias in observational studies. Sankhya 35, 417–446.
Czarnitzki, D., 2006. Research and development in small and medium-sized enterprises: the role of financial constraints and public funding. Scottish Journal of Political Economy 53 (3), 335-257.
Czarnitzki, D., Hottenrott, H., 2011. Financial constraints: routine versus cutting edge R&D investment. Journal of Economics and Management Strategy 20 (1), 121–157.
Czarnitzki, D., Hottenrott, H., Thorwarth, S., 2011. Industrial research versus development investment: the implications of financial constraints. Cambridge Journal of Economics 35 (3), 527–544.
Czarnitzki, D., Hussinger, K., 2004. The Link between R&D Subsidies, R&D Spending and Technological Performance, ZEW Discussion Paper No. 04-56, Mannheim.
Czarnitzki, D., Licht, G., 2006. Additionality of public R&d grants in a transition economy: the case of eastern Germany. Economics of Transition 14 (1), 101–131.
Czarnitzki, D., Lopes-Bento, C., 2011a. Evaluation of Public R&D Policies: A Cross-Country Comparison, World Review of Science, Technology and Sustainable Development, forthcoming.
Czarnitzki, D., Lopes-Bento, C., 2011b. Innovation Subsidies: Does the Funding Source Matter for Innovation Intensity and Performance? Empirical Evidence from Germany, ZEW Discussion Paper No. 11-053, Mannheim.
Czarnitzki, D., Toole, A.A., 2007. Business R&D and the interplay of R&D subsidies and product market uncertainty. Review of Industrial Organization 31 (3), 169–181.
Czarnitzki, D., Toole, A.A., 2011. Patent protection, market uncertainty, and R&D investment. Review of Economics and Statistics 93 (1), 147–159.
David, P.A., Hall, B.H., Toole, A.A., 2000. Is public R&D a complement or substitute forprivate R&D? A review of the econometric evidence. Research Policy 29 (4–5), 497–529.
Davidson, R., MacKinnon, J., 1981. Several tests for model specification in the presence of alternative hypotheses. Econometrica 49, 781–793.
Dehejia, R.H., Wahba, S., 1999. Causal effects in nonexperimental studies: reevaluating the evaluation of training programs. Journal of the American Statistical Association 94, 1053–1062.
Dixit, A.K., Pindyck, R.S., 1994. Investment Under Uncertainty. Princeton University Press, Princeton, NJ.
Duguet, E., 2004. Are R&D subsidies a substitute or a complement to privately funded R&D? Evidence from France using propensity score methods for non-experimental data. Revue d'Economie Politique 114 (2), 263–292.
González, X., Jaumandreu, J., Pazó, C., 2005. Barriers to innovation and subsidy effectiveness. RAND Journal of Economics 36 (4), 930–949.
Griliches, Z., Mairesse, J., 1984. Productivity and R&D at the firm level. In: Griliches, Z. (Ed.), R&D, Patents and Productivity. University of Chicago Press, Chicago, IL, pp. 339–374.
Griliches, Z., 1990. Patent statistics as economic indicators: a survey. Journal of Economic Literature XXVIII, 1661–1707.
Hall, B.H., 1990. The impact of corporate restructuring on industrial research and development. Brooking Papers on Economic Activity (1), 85–136.
Hall, B.H., 2002. The financing of research and development. Oxford Review of Economic Policy 18, 35–51.
Hall, B.H., Lerner, J., 2010. The financing of R&D and innovation. In: Hall, B.H., Rosenberg, N. (Eds.), Handbook of the Economics of Innovation, Vol. 1. Elsevier, Amsterdam, pp. 609–639.
Hall, B.H., Maffioli, A., 2008. Evaluating the impact of technology development funds in emerging economies: evidence from Latin America. European Journal of Development Research 20 (2), 172–198.
Hall, B.H.J., Van Reenen, 2000. How effective are fiscal incentives for R&D? Research Policy 29, 449–469.
Heckman, J.J., Ichimura, H., Todd, P., 1997. Matching as an econometric evaluation estimator: evidence from evaluating a job training program. Review of Economic Studies 64 (4), 605–654.
Heckman, J.J., Ichimura, H., Todd, P., 1998a. Matching as an econometric evaluation estimator. Review of Economic Studies 65 (2), 261–294.
Heckman, J.J., Ichimura, H., Smith, J.A., Todd, P., 1998b. Characterizing selection bias using experimental data. Econometrics 66, 1017–1098.
Heckman, J.J., Lalonde, R.J., Smith, J.A., 1999. The economics and econometrics of active labour market programs. In: Aschenfelter, A., Card, D. (Eds.), Handbook of Labour Economics, 3. Amsterdam, pp. 1866–2097.
Hussinger, K., 2008. R&D and subsidies at the firm level: an application of parametric and semi-parametric two-step selection models. Journal of Applied Econometrics 23, 729–747.
Imbens, G.W., Wooldridge, J.M., 2009. Recent developments in the econometrics of program evaluation. Journal of Economic Literature 47, 5–86.
Jaffe, A.B., 1986. Technological opportunity and spillovers of R&D: evidence from firm's patent, profits, and market value. American Economic Review 76 (5), 984–1001.
Lamberton, D.M., 1996. 'Introduction: 'Threatened Wreckage' or New Paradigm?'. In: Donald, M.Lamberton (Ed.), The Economics of Communication and Information. Cheltham UK and Brookfield US: Edward Elgar, pp. xiii–xxviii.
Larosse, J., 2004. Conceptual and Empirical Challenges of Evaluating the Effectiveness of Innovation Policies with 'Behavioural Additionality' (The Case of IWT R&D Subsidies). IWT Studies 48, 57–69.

Lechner, M., 1999. Earnings and employment effects of continuous off-the-job training in East Germany after reunification. Journal of Business and Economics Statistics 17, 74–90.
Lechner, M., 2000. An evaluation of public sector sponsored continuous vocational training in East Germany. Journal of Human Resources 35, 347–375.
Lechner, M., 2001. Identification and estimation of causal effects of multiple treatments under the conditional independence assumption. In: Lechner, M., Pfeiffer, F. (Eds.), Econometric Evaluation of Labour Market Policies. Physica, Heidelberg, pp. 43–58.
Nelson, R.R., 1959. The simple economics of basic scientific research. Journal of Political Economy 49, 297–306.
OECD, 1993. The Proposed Standard Practice for Surveys of Research and Experimental Development – Frascati Manual, Paris.
OECD/Eurostat, 2005. Guidelines for Collecting and Interpreting Innovation Data – the Oslo Manual, 3rd edition, Paris.
Rosenbaum, P.R., Rubin, D.B., 1983. The central role of the propensity score observational studies for causal effects. Biometrica 70, 41–55.
Rubin, D.B., 1977. Assignment to treatment group on the basis of covariate. Journal of Educational Statistics 2, 1–26.
Schumpeter, J., 1942. Capitalism, Socialism and Democracy. Harper and Row, New York.
Smith, J.A., Todd, P.E., 2005. Does matching overcome LaLonde's critique of nonexperimental estimators? Journal of Econometrics 125, 305–353.
Takalo, T., Tanayama, T., Toivanen, O., 2012. Estimating the benefits of targeted R&D subsidies, Review of Economics and Statistics, forthcoming.
Wallsten, S.J., 2000. The effects of government-industry R&D programs on private R&D: the case Small Business Innovation Research Program. RAND Journal of Economics 31 (1), 82–100.